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THE JOURNAL OF PHILOSOPHY
PSYCHOLOGY AND SCIENTIFIC METHODS

ON THE NATURE OF INDUCTION[1]

ANY proposition is susceptible to two sorts of proof. We can adduce premises that directly imply it, or we can adduce premises that indirectly imply it because they imply the falsity of its contradictory alternatives. In inductive reasoning we prove universal propositions by adducing as premises the particular propositions furnished by experience. Formal logic tells us that the value of a particular proposition consists in its power to disprove its contradictory universal rather than to prove its subalternate universal. We might naturally suppose that the evidential function of experience as a knowledge of particulars, was to disprove universal statements rather than to prove them, and that if a universal conclusion was proved true by appeal to experience, the proof would be based upon the disproof or elimination of alternatives. That induction is actually and always of this indirect type of inference, and that as such it is properly expressed by a disjunctive syllogism in the negative mood (*modus tollendo ponens*), is what I wish to show.

There is, of course, no novelty in the conception of induction as a process of elimination. Mill's canons are efficacious because they embody implicitly the eliminative principle. In Hobhouse and Aikins, to mention only two of the modern logicians, the principle is explicitly recognized, and the chief problems of induction are treated, especially by Hobhouse, from that point of view. Yet so far as I am aware there has been nowhere an attempt to identify induction in all its phases with the kind of indirect inference known as the *reductio ad absurdum,* and it has seemed to me worth while to make that attempt for two reasons: First, because the several inductive methods when viewed from this standpoint appear not as a group of disconnected principles, but as an organic system and hierarchy which is applicable in its entirety to every inductive problem and in which each principle has its own function and virtue by which it

[1] Read at the annual meeting of the American Philosophical Association, at Cambridge, December, 1905.

supplements the defects of the principles that precede it; second, on account of the new light thrown by the indirect theory of induction upon the general epistemological problem of deriving universals from particulars.

And now, by way of introduction to the more positive treatment of the subject, let us consider some of the difficulties involved in what is still, I think, the usual conception of induction. Induction when treated as a mode of direct inference is divided into two kinds, —perfect and imperfect. In perfect induction, we reason that as these *A's* are *B's* and as these *A's* are all the *A's*, it must follow that all *A's* are *B's*. It is clear that what is called perfect induction is only possible when the total number of individuals making the class is limited. Thus we can prove by this method that all the months in the year have less than thirty-two days, or that all the flowers in the garden are fragrant, but not that all bodies gravitate or that all men are mortal. In these latter propositions, which are genuine universals, the classes contain an unlimited number of members, and experience can never supply us with more than an insignificant fraction of them. In imperfect induction, which is supposed to be a degenerate form of perfect induction, we boldly conclude that because an infinitesimal portion of a class has been observed to possess a certain property the whole class will have that property. The methods or canons of induction are the principles that inform us when we can and when we can not take the inductive leap.

Now there is one circumstance in particular which might lead us to suspect that there was something radically wrong with the notion that induction is a degenerate form of perfect induction. Neither the actual number of positive instances observed nor the ratio of that to the total number has anything whatever to do with the degree of validity possessed by the induction. Perfect induction is essentially quantitative, depending, as it does, upon observation of all the members of a given class. The canons that guide us in making the so-called imperfect induction are, on the other hand, essentially qualitative, and not, as we might suppose, imperfectly quantitative. That is to say, it is never a question of observing almost all, or a bare majority, or even an appreciable fraction of the whole number of material bodies, for example, as evidence for the inductive generalization that all bodies gravitate. We contrive in the few cases under our control to eliminate by the methods of difference and especially of agreement all the characteristics of bodies that could possibly cause their gravitation except those of extension and inertia, and on the strength of this elimination we unhesitatingly conclude that a material body, merely as such (and hence all material bodies), will gravitate.

And now that we have briefly considered the contradiction between inductive theory as exemplified in the supposedly archetypal syllogism of perfect induction, and inductive practise as exemplified in Mill's canons, we may look to see how this contradiction can be removed by treating induction as belonging essentially and exclusively to the indirect type of inference.

Every inductive problem indirectly, and the usual inductive problem directly, concerns the determination of a causal relation. A phenomenon occurs in which we are for some reason interested and we at once seek among its antecedents and consequents for phenomena which are related to it as cause and as effect. Defining a causal relation as the relation of universal concomitant presence, absence and variation of two phenomena, we must assume as the basal postulates of all induction (1) that every event has an antecedent and a consequent with which it is causally or universally related, and (2) that we can enumerate these possible causal relations, by the aid of perception and previous knowledge. Now let M be a phenomenon whose causal relations we are seeking to discover, and let A be an antecedent or consequent phenomenon which we suspect, or provisionally assume, to be, and which in reality is, causally related to M; we can then classify the possible causal relations of M with respect to A under five heads. This division may be briefly stated in the form of a disjunctive proposition which will constitute the major premise of a typical inductive syllogism. Thus we can say that

The cause or effect of M is either (1) a phenomenon symbolized by X that is related to A only casually or by chance; or (2) a phenomenon symbolized by B, C or D, which is collocated with A but not indissolubly; or (3) a complex phenomenon symbolized by $A\,B$, $A\,C$ or $A\,D$, of which A is an indispensable part; or (4) a phenomenon symbolized by a which is an aspect, phase or degree of A; or (5) A itself. The four inductive methods of simple enumeration, difference, agreement and concomitant variation express the types of particular negative propositions furnished by experience, and as such they constitute the complex minor premises of the syllogism and serve to contradict or eliminate all but one of the alternatives set forth in the major premise. The conclusion is, of course, the categorical affirmation of the only alternative not eliminated. I shall now try to show how each of the inductive methods is especially suited to eliminate one of these alternatives and that the eliminative function is the only function that they could or do perform: First, then, to remove the possibility that A and M are connected merely by chance, we use the method of simple enumeration. We observe the frequency with which M occurs in conjunction with A, and compare this with the frequency with which M might be expected to occur

with A if they were quite independent. If the former frequency greatly exceeds the latter we consider the conjunction to be something more than casual. The *number* of observations required to eliminate the hypothesis of chance is thus strictly determined by the joint independent probability of the events in question.

Having eliminated chance by the method of simple enumeration, we next eliminate by the method of difference the possibility that M is causally related, not to A, but to the antecedents and consequents with which A is collocated. We find, let us say, cases in which B, C and D are simultaneously or successively present when the event M is absent. The results of such observations may be stated in the form of a particular negative proposition, as follows: Some cases of B, C and D are not cases of M, which means that the universal affirmative proposition: All cases of B, C or D are cases of M—which expresses a possible causal relation—is eliminated.

Supposing, now, that the phenomenon A has not been found present in the absence of M, and consequently has not been eliminated, it becomes necessary to apply the method of agreement in order to decide whether M is not causally related to a complex phenomenon such as AB, AC or AD, of which A is only a part, for the weak point in the method of difference lies in the fact that it can only prove that A is at least a part of the cause or effect of M, not that it is the whole. We observe by the method of agreement that B, C and D can be simultaneously or successively absent when M and A are both present. This again eliminates B, C and D, but it also eliminates the possibility that A needs to cooperate with B, C and D in order to be causally related to M.

We have now proved that M is causally related either to A or to some phase of A which we called a. And to secure the elimination of this fourth alternative we use the last and most powerful of the inductive canons, *viz.*, that of concomitant variation. If we find that M and A vary in perfect concomitance we know that every phase or degree of A, rather than some particular phase such as a, is causally related to every phase or degree of M. If, on the other hand, we had discovered that A did not vary with M in any manner, we should have proved that the true cause or effect of M was a and not A, as such. Or, again, if we had found that M did not vary directly with A, but with some function of A, we should conclude that the cause or effect of M was A in conjunction with a.

The universal affirmative conclusion of an inductive syllogism is thus in any case the result of successive eliminations—in the form of the particular negative propositions furnished by experience—of all but one of the alternative universals set forth in the disjunctive major premise as hypotheses. And each of the inductive methods is,

as we have seen, adapted to the elimination of a certain type of alternative.

And now a word must be said in regard to the two methods which we have not mentioned—the method of residues and the joint method of agreement and difference. The method of residues is confessedly a method of elimination; it is, however, hardly worthy of being ranked with the other methods, for it is applicable only when both the antecedents and consequents are quantities of matter or energy, for in no other case can we apply the conception of a cause exhausting its causality in the production of a given effect. As for the joint method, its continued existence in logical text-books affords a good illustration (1) of the fact that logicians have failed to recognize the exclusively eliminative nature of induction and (2) of the results of that failure. The joint method bids us supplement the method of agreement, by the collection of as many different instances as possible of the absence of a phenomenon along with the absence of its supposed cause. Now it can easily be shown that these cases of concurrent absence are as such quite worthless as evidence of causal connection. If we are considering whether a protective tariff causes national prosperity we do not adduce as evidence the generation of Roentgen rays or the constructing of a sonnet, and yet these are different cases of the concurrent absence of protection and prosperity and as such perfectly conform to the requirements of the joint method as worded by Mill and as symbolized by Jevons. What we actually seek to find in such an investigation are always cases in which not merely the supposed cause is absent, but in which *the alternatives to the supposed cause are present* along with the absence of the effect, and hence are eliminated. In the method of difference as usually schematized this is done in a single pair of instances in which *ABC* followed by *M* is compared with *BC* followed by the absence of *M;* but it can equally well be done piecemeal or by a succession of instances, one showing simply the presence of *B,* a second the presence of *C,* a third the presence of *D,* etc.; along with the absence of *M.* Now as the so-called joint method is, when rightly understood, nothing whatever but a combination of the method of agreement and the method of difference where each is applied successively in several instances, rather than simultaneously in a single pair, it does not deserve to be classed as a separate canon.[2]

The claim was made at the beginning of the paper that the identi-

[2] By way of illustration I subjoin (1) the incorrect and meaningless symbolization of the joint method given by Jevons, 'Lessons in Logic,' p. 247; (2) the correct symbolization substantially as given by Aikins, 'The Principles of Logic,' p. 275; (3) the symbolization in a single pair of instances of the

fication of induction with the indirect type of argument, or *reductio ad absurdum*, possessed two advantages, (1) the unification of the inductive methods, (2) the exhibition in a new light of the general epistemological problem of deriving universals from particulars. I have said what I could in regard to the first of these advantages, but I should like in conclusion to speak further as to the second.

The attacks upon the possibility of reasoning from particulars, that have been made by the sceptics on the one hand and by the extreme apriorists on the other, are based in the main upon a quite proper realization of the gulf between a subaltern proposition and its subalternates. The number of cases exemplifying a genuine universal or law of nature is, as we have said, always infinite, and hence the direct inference from some to all is not only uncertain (which would be admitted by inductive logicians), but would seem to be not even probable—to be, in fact, infinitely improbable. For we can, of course, never observe even an appreciable fraction, to say nothing of a majority of the members of an infinite series. Now when we give up this attempt at direct inference and exorcise from inductive theory the specter of a so-called perfect induction as an ideal to be approximated, the whole problem appears in a less paradoxical and more hopeful light. For the experiential evidence in the form of particular propositions, which was worthless as a means of direct proof of the subalternate, is perfectly capable of disproving the contradictory and thus indirectly establishing a hypothesis as a survival of the fittest. Of course this does not mean that we have merely, by the substitution of the intensive for the extensive view of the subject, removed uncertainty from generalizations from experience, but only that from our point of view we may more clearly see why it is that the degree of probability of any inductive conclusion is measured by the number of antecedently possible alternative conclusions and by the ease with which they can be isolated, enumerated and eliminated rather than by the mere number of instances observed.

W. P. MONTAGUE.

COLUMBIA UNIVERSITY.

method of agreement and of the method of difference. The substantial identity of (2) and (3) is obvious.

(1)	(2)	(3)
ABC — abc	*ABCD — abcd*	*ABC — abc*
ADE —ade	*ABGH — abgh*	*ADE — ade*
AFG — afg	*ACGK — acgk*	
AHK — ahk		
PQ — pq	*BDE — bde*	*ABC — abc*
RS — rs	*CDG — cdg*	*BC — bc*
TV — tv	*BEF — bef*	
XY — xy		

THE FINAL EDITION OF SPENCER'S 'FIRST PRINCIPLES: PART I'

WHEN in 1896 Mr. Spencer had finished the task he laid out for himself in 1860, he set out to do what the more important work of completing his system had hitherto prevented, namely, to revise the earlier parts of the synthetic philosophy. The revised edition of the 'Principles of Biology' appeared in 1898–9, and of the 'First Principles' in 1900. Declining strength and health prevented any further work on the revision.

The changes made in the 'First Principles' are exceedingly numerous, and some of them of considerable importance. Nevertheless but little attention has been paid to them, in spite of the significance of the work and the influence which it has exerted. The present writer has made a complete collation of the fourth edition, 1880, with the sixth, definitive edition of 1900, the alterations introduced in the fifth edition, 1884, being few in number and negligible in character.[1] In this paper the results of the investigation are given so far as they concern the variations in Part I., 'The Unknowable.' The discussion of the variations in Part II. will be given in a later article. Though it has been often regarded as the basis for the acceptance or rejection of Spencer's system as a whole, Part I.,—as Mr. Spencer himself points out, both in the new 'Postscript' to Part I. and in his 'Autobiography,'[2]—is an unessential part of the synthetic philosophy. This may account in part for the fact that, though it forms obviously the weakest part of the system and has been subjected to the most searching criticism, through all the changes which were made in the 'First Principles' in 1866 and 1875 Part I. received but two trifling verbal alterations. And even in the revised edition of 1900 the importance of the changes made in Part I. is notably less than of those in Part II. This does not imply, however, that Spencer did not make many changes: rather the enormous number of them is the first thing that attracts the attention. There is hardly a paragraph, indeed on some pages not a sentence, that has not been cut into or rearranged. In the vast majority of instances—and the same is true of Part II. as well—the alterations are stylistic. Spencer says in the preface (p. vi), "While the changes of substance in this edition constitute improvements of some significance, the changes of form constitute a greater general im-

[1] For material assistance in the investigation and in the preparation of this paper, the writer is indebted to Professor A. C. Armstrong, of Wesleyan University.

[2] Vol. II., p. 86.

provement." Altogether the largest number are due to a desire to eliminate redundancies, so that, notwithstanding considerable additions, the size of the 'First Principles' is reduced by fifty pages, of which Part I. loses seventeen. The whole forms a good example of Mr. Spencer's method of literary work as it is described in the 'Autobiography': "So far from disliking the process of polishing as most writers do, I had a partiality for it and can not let any piece of work pass so long as it is possible to improve it."[3] And speaking more particularly of the 'Study of Sociology': "Every sentence in the work had passed under my eye for correction five times, and each time there was scarcely a page which did not bear some erasures and marginal notes."

The second class of changes, which, however, Mr. Spencer mentions first in the preface, are those due to advances made in science since the book was first written. An example of these is found in the removal of the argument drawn from philology against the origin of religious ideas before the dispersion of mankind.[4] Such a negative statement later philology would not attempt; rather it has brought forth arguments to prove that so far as its evidence goes such an origin is possible. Among the rare additions is a foot-note on the subject of Lord Kelvin's hypothesis of vortex-atoms;[5] this is introduced in connection with the discussion of the incomprehensibility of matter. Kelvin's theory is dismissed, along with those of Newton and Boscovich, as involving elements which 'can not be truly represented in consciousness.' An obvious correction, which should have been made long before, is the suppression of the following sentence, "The influence conveyed through the nerves to the muscles is, though not positively electric, yet a form of force nearly allied to the electric."[6] Corrections due to scientific changes, however, are naturally much fewer in Part I. than in Part II., since the subject-matter involves fewer scientific illustrations.

A third class of alterations is described in the preface as due to further developments of the author's own thoughts. This class is well illustrated by two substitutions in the section which bases the argument for relativity on Spencer's celebrated definition of life. The sentence, "Divesting this conception of all superfluities and reducing it to its most abstract shape, we see that life is definable as the continuous adjustment of internal relations to external relations," becomes in the sixth edition, "So that passing over its noumenal nature, of which we know nothing, life is definable as the

[3] Vol. II., p. 423.
[4] 4th ed., p. 14.
[5] 6th ed., p. 46, note.
[6] 4th ed., p. 72.

continuous adjustment of internal relations to external relations.''[7] Similarly, ''If, then, life in all its manifestations, inclusive of intelligence in its highest forms, consists in the continuous adjustment of internal relations to external relations, the necessarily relative character of our knowledge becomes obvious,'' is changed to, ''If, then, life as knowable to us, inclusive of intelligence in its highest forms, consists, etc.''[8] These changes evidently refer to the doctrine set forth at greater length in a new chapter in the revised edition of the 'Principles of Biology,' 'The Dynamic Element in Life,'[9] a chapter which has received all too little attention. In this Mr. Spencer acknowledges the inadequacy of his former definition of life as the continuous correspondence between inner and outer relations. It recognizes, he argues, in self-criticism, only the *form*[10] of our conception of life, and that incompletely, while it ignores the *body*[10] of life altogether. The essential element has been neglected in the earlier discussion. When it is said that life consists in the continuous maintenance of relations, the question must be asked, relations between what things? For a relation of which the terms are unspecified gives us no true thought, but only the blank form of thought. Some principle of activity, some dynamic element, must be the substance and essential part of our idea of life. Then, after arguing that life can not be conceived either as a vital principle, a *vis vitae*, or in physicochemical terms, he adopts as the only possible alternative that it is another inexplicable manifestation of the unknown and ultimate reality,—a conclusion that is not only dubious philosophically, but questionable from the scientific standpoint as well.

In connection with the further development of Spencer's thinking some notice should also be given of the apparent softening of statement which at times accompanies the improvement in diction. References to 'inexorable logic' almost disappear, as do many strong adjectives and adverbs, such as 'absolutely,' 'positively,' 'rigorously,' 'inevitably' and the like, of which he formerly made abundant use. 'And this assumption is made by the immense majority of philosophers, past and present,' becomes more simply—'most philosophers.'[11] But the illustrative passages are too numerous to cite. The question naturally arises, is this variation in statement accompanied by or indicative of any softening of doctrine, any change of feeling towards religion, for example, or mitigation of his

[7] 4th ed., p. 84; 6th ed., p. 70.

[8] 4th ed., p. 85; 6th ed., p. 72.

[9] 'Principles of Biology,' 1898, Vol. I., Part I., Chap. VI. A.

[10] Original italics.

[11] 4th ed., p. 33; 6th ed., p. 27.

agnosticism? Such a substitution as 'rudest beliefs' for 'grossest superstitions,' perhaps does indicate a calmer attitude towards religion, a modification like that recorded in the pathetic 'Reflections' that close the 'Autobiography.'[12] But any change in his agnosticism, in spite of the fact that nearly one half of the passages containing the words 'Inscrutable Power' or 'the Unknowable' have been expunged from Part I., must be denied. The changes have been made equally on the positive and the negative side of the discussion. The crucial passages in the chapters on the 'Relativity of All Knowledge' and the 'Reconciliation' all remain and are reinforced in the new 'Postscript.' To the question, then, did Spencer moderate his agnosticism, we must give a decidedly negative answer.

The principal addition to Part I., the 'Postscript,' is especially mentioned in the preface as coming under this class of changes. In it Mr. Spencer notices what he terms the two chief criticisms which Part I. has called forth:—that it is illegitimate to assert of the ultimate reality that it is unknown and unknowable, first, 'as putting an arbitrary limit to human faculty,' and second, as involving a contradiction by 'asserting something concerning that of which we are said to know nothing.' The first criticism, it is replied, had already been sufficiently answered in §§ 24 and 25; thinking being relationing, any knowledge of the absolute is excluded, not arbitrarily, but by the very nature of thought. But the second criticism is acknowledged to contain greater difficulties, since it is true 'that saying what a thing is not, is in some measure saying what it is,' as is illustrated in the game of Twenty Questions. So that 'it can not be denied that to affirm of the ultimate reality that it is unknowable is, in a remote way, to assert some knowledge of it, and therefore involves a contradiction.' And yet, it is held that this very contradiction itself shows the impossibility of any knowledge beyond phenomena. Intellect being capable of dealing only with phenomena, involves us in contradictions just as soon as we attempt to use it for anything beyond phenomena. Not only so, but we can not complete any such process of thought, nor even conceive any connection between a noumenon and a phenomenon, for here one of the terms of the relationing which constitutes thought is a blank, and 'such a relation is not truly imaginable.' This last clause deserves to be emphasized. For it shows that Mr. Spencer has never advanced beyond his initial error: he still fails to distinguish between thinking and imaging. Nevertheless, after thus affirming that thought concerning the infinite is impossible, in the next paragraph he argues, "Yet by the very nature of our intelligence we are compelled continually to ascribe the effects we know to some cause

[12] Vol. II., pp. 544–549.

which we do not know,—to regard the manifestations we are conscious of as implying something manifested.'' Then he asserts once more that the endeavor to think a relation between appearance and reality, though foiled, implies the existence of such a reality; and complains that his critics have overlooked the distinction he makes. ''Their arguments,'' he says, ''are directed against one or other element in a *conception* which they ascribe to me; forgetting that, equally with them, I deny the possibility of any conception, and affirm only that after all our futile attempts to conceive there remains the indefinable substance of a conception—a consciousness which can not be put into any shape.'' Again, a knowledge that is not knowledge at all, but something between that and nescience, is made superior in positiveness and necessity to real knowledge. In fine, the remarkable thing is not the change in the thought of Mr. Spencer, but the absence of change.

But the 'Postscript' ends by pointing out the truth which the author saw clearly,[13] but which has escaped the attention of many of his readers, namely, that the value of Part II. is quite independent of Part I. The latter was written merely with the intention of preventing any misunderstanding of his purpose in the synthetic philosophy, lest any one might think he was propounding a purely materialistic philosophy.[14] Understanding that this is not his intention, the reader is at liberty to reject or adopt so much as he likes of it, without thereby rejecting or adopting the conclusions of Part II. And in this, of course, Spencer is abundantly right.

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DISCUSSION

FINAL STATEMENTS IN THE DISCUSSION BETWEEN PROFESSOR MINER AND DR. BAIRD

IN the controversy[1] which has arisen between Dr. J. W. Baird and myself over my investigation of 'A Case of Vision Acquired in Adult Life,'[2] Dr. Baird has made several statements in his recent reply which, it seems to me, should not be left unanswered.

One of these statements suggests that there may be a misunderstanding among psychologists as to what is normal pitch discrimina-

[13] Cf. above, p. 287.

[14] Cf. 'Autobiography,' II., p. 86.

[1] This JOURNAL, Vol. II., p. 692; Vol. III., pp. 45, 101.

[2] *Monograph Supplement, Psychological Review*, VI., pp. 103–118.

tion. In my paper I said regarding Miss. W.: "Although she has this wide range of tone sensations, we found that her discrimination between simple tones was not unusually keen. With the tuning-forks she distinguished, nine times out of ten, a difference of eight vibrations from the international *a'* (435 vibrations.)" Dr. Baird in his review said: "Other investigators have found that the normal difference for this region of the tonal scale is less than .3 vibration; Dr. Miner confined [?] his tests to a stimulus difference of eight vibrations and naïvely remarks that his patient's discrimination of simple tones was not unusually keen." In my reply to the review, I referred him to the *University of Iowa Studies in Psychology*, II., p. 56, published in 1899, where data were given on 19 women. This group showed an average discrimination of 9 vibrations, a median of 8. In his answer to this reference Dr. Baird said as follows:

"Dr. Miner replies by introducing *new*[3] data which *he*[3] has obtained from normal subjects, and which he finds to bear witness to the normality of his patient. Why were these data not included in the original paper, to which they properly belong? It is just this failure to present comparative results which has been a chief factor in rendering the publication abortive. Moreover, the fact that the author's determinations of the normal difference limen for tones are twentyfold greater than the normal determinations of the most reliable workers in the field of psychological acoustics shows that there is something radically wrong with Dr. Miner's conditions of practise or of experimentation." I would call attention to the facts that the data were not new, were referred to by page reference, were not obtained by me, and showed that it is possible to make at least a rough comparison with results on normal groups found by other investigators.

As to the literature on pitch discrimination, Spearman[4] has pointed out the contrast between the records that have been announced by Delezenne, Siebeck,[5] Preyer, Luft, Meyer, etc., in tests made mainly on themselves, and the norm for groups of individuals published by Cattell and Farrand, Wissler, Seashore, Gilbert, Myers and Spearman. Although the acute and trained ears of the expert investigators of acoustics may attain a discrimination of .3 vibration, such results are not comparable with records on groups of individuals. I tested Miss W. without practise to make the records easily comparable with a general group. Spearman gives a table which he says 'appears to bring the various results of the best

[3] Italics mine.

[4] *American Journal of Psychology*, XV., p. 228 ff.

[5] For references not given by Spearman, see Titchener's 'Instructors' Manual, Quantitative,' p. 235 ff.

workers into very good harmony with one another and also with my own.' The table suggests that, with a minimum of fore-exercise, non-musicians of general culture discriminate normally 10 vibrations; musicians, 4 vibrations; specially practised reagents, .5 vibration. After fifteen minutes of good fore-exercise, non-musicians of general culture dicriminate normally 4 vibrations; musicians, 2 vibrations; specially practised reagents, $\frac{1}{3}$ vibration. Spearman's tests, made with a monochord, with a maximum of fore-exercise, show a median of 3 vibrations for 27 adults, and of 7 vibrations for 24 children (11–13 years). A group test given simultaneously on 83 children (9–13 years) gave a median of 11 vibrations. Myers, using tuning-forks, gives records on 18 adults, without fore-exercise, which show a median of 5 vibrations. The Columbia records, obtained on a monochord, published by Wissler, show an average discrimination for a group of 265 men students, 17 vibrations; 42 women students, 11 vibrations. The above records were for tones near the middle of the scale where the absolute pitch of the standard makes little difference. From the published results we may, therefore, suppose that practise is so important as to make the most available method for comparing an individual with a group to be a test with a minimum of fore-exercise. This was the form of test given Miss W.

As for records taken after the limit of practise has been reached, we have none for a group of any size, so far as I have been able to discover. Titchener, in his quantitative manual for instructors, gives the Cornell laboratory records for his experiment on pitch discrimination to be as follows: "The value of *DL* (absolute) as observed under the described conditions has never exceeded 2 vibrations for either set of forks, and has fallen as low as .75 vibration." But, he says of the laboratory records that they 'represent only a certain stage of psychological training and are intrinsically valueless whether for theory or for practise' (124). Whipple has published records which show the effect of practise on three or four subjects who discriminated poorly at first. None of these records, it seems to me, justify Dr. Baird's claim that 'the normal difference limen for this region of the tonal scale is, after preliminary practise, considerably less than one vibration.' What the norm for a group might be, after the limit of practise, has not yet been determined. Without practise, we are sure that it is considerably above one vibration.

Dr. Baird refers to the literature to corroborate another contention. He admits that he should have used the word 'cataracts' instead of describing the patient as 'a young lady who had a cataract removed'; yet he believes that he was justified in the use of the singular when she had two removed. He says: "If Dr. Miner will consult the literature he will find that I have abundant authority

for the form of expression which I employed. Would he maintain that Ware, Franz and other writers of equal prominence have persistently 'misrepresented' the condition of their patients?'' In rereading the reports of Ware and Franz[6] I find that Ware does mention *the operation* as an operation for 'the cataract.' But, if I read aright, neither Ware nor Franz describes *'the condition of his patient'* as having 'a cataract.' On the contrary, they use 'cataracts' or 'cataract of both eyes' when telling about their patients. Moreover, it is to be noted that these cases bring out the importance of the distinction which Dr. Baird neglected in his description of Miss. W. The patient mentioned in the title of each of these English reports was successfully operated on for cataract in only one eye. Miss W. had a complete congenital cataract removed from each eye.

I regret that Dr. Baird and I did not use 'keenness' of vision with the same meaning. It has led to his making the unpleasant charge that I criticized him for an omission he did not make. I called attention to the fact that, among other important omissions, he completely overlooked the summarized conclusion that 'color vision was abnormally keen.' His sole reference to color vision in his review was to mention that the patient's 'spectrum is about one fifth longer than the average of ten students.' By keenness of color vision, I referred to color *discrimination.* This was stated at the conclusion of my paper as follows: ''Color vision is so far above normal as to contradict any supposition that twenty-two years of disuse would cause degeneracy. On the contrary, either the color process deteriorates with use or the removal of the lens and unusual interest produce a remarkable ability to discriminate colors.''

Something must have been wrong with my presentation of the facts or Dr. Baird would not have discussed, as a test for measuring the perception of depth, an experiment which was to determine whether Miss W. was actually able to use both eyes together. From the medical standpoint, it was important to know whether an adult could really attain single binocular vision. Such an experiment was largely qualitative in its nature. In the middle of a paragraph where I was discussing this subject of single binocular vision, Dr. Baird found my statement about a test to compare the subject's single and binocular vision. Because I said 'her accuracy doubled' when she used both eyes in judging the relative distance of two balls, he was led to suppose that I was interested in measuring her perception of depth. My form of expression was employed to emphasize that she was actually using a different method, and not merely neglecting the image on one retina, when she thought she was using both eyes. While the detailed description of the test, which is yet to be

[6] 'Transactions of the Royal Society,' 1801, 1841.

published, might justify the test as one for the perception of depth, such a test was not planned. It would give no basis for comparison with a normal group, unless it could be supplemented by an extended investigation of the secondary factors which Dr. Baird mentions. From the fact that the subject had no lens in either eye and used spectacles, such an inquiry was wholly beyond the scope of what I could expect to do. The problem whether or not she could use both eyes together in judging distance was the important general question to be determined.

While I am still struggling with the practical question whether I shall be able to make further tests on Miss W., and whether the promise of other results makes further experiments advisable, Dr. Baird seeks to pierce me with one or the other horn of a logical dilemma—"Was the patient available for additional experiments or was she not?" When the experiments began, I thought she would be available for three days, then for a week, then for a longer time. Unfortunately, I am not very sure yet what the answer to his question is, except that it is not unconditional. I published the preliminary report, hoping that it would call forth suggestions from those whose special work might be definitely reached by this case. Dr. Baird has given me the suggestion that I test her limen for pressure and also for twoness, which I postponed for practical reasons. I now hope to have Miss W. at the university for a few days the first of May, and I shall be glad to receive any other suggestions before that time.

In the conduct of my investigation, the effect of the disuse of sight on the subject's other senses was a wholly subordinate matter. Only *one page* of my report was devoted to tests on other senses than sight. My effort was directed to the study of the subject's vision and her learning process. In the discussion in this JOURNAL, altogether too much emphasis has been placed on tests of hearing and touch. Dr. Baird, in his review, mentions that these were 'incidental,' yet he devoted to them about one half of his page-review of the paper. He did this, no doubt, because he felt that the tests, or the description of them, needed criticism. Whether, in general, the loss of sight is compensated by increased capacity in other senses is an interesting question on which the case has a bearing. Where Miss W. might rank with a normal group would be, however, only an introduction to the problem of how a group of blind people would compare with a group having sight. Through the discussion of the incidental problem of compensation, Miss W.'s eyesight has been left in the background, although it is of vastly more importance in this case. In my report I tried to emphasize certain rather surprising results in the field of sight, such as the reversal of irradiation, the

patient's remarkable color vision, the absence of retinal rivalry, the unexpected attainment of single binocular vision by an adult, the development of the perception of number, etc. The main subject of the investigation was necessarily the acquisition of *vision*.

In his review Dr. Baird severely criticized the incompleteness of my investigation and the absence of normal records for comparison; but he gave the reader no intimation that my paper was declared to be 'a brief preliminary report.' Moreover, I stated in it that 'before the more complete technical description of the experiments and the quantitative results are published, it is desirable that the tests should be repeated on a representative group of normal adults.' Because of the bad impression this omission from his review would create, I believe it was misleading. Because he used the absence of *systematic* experiments in passive touch as an illustration of 'haphazard and inaccurate' tests, I suggest that he further confused incompleteness with lack of thoroughness. Because he said that the results of a test on active touch were 'unmeaning' when normal records for comparison were not given in the paper, I question his criticism. Because he compared the pitch record of Miss W. with that of long-practised experts, I take the opportunity of correcting a possible misconception about normal pitch discrimination. Because he supposed that I tried to quantitatively determine the subject's perception of depth when I was studying whether she acquired single vision with two eyes, I defend myself. Because he misrepresented by describing the patient as having 'a cataract removed,' I corrected him. Because the problem of compensation has become over-emphasized, I plead for a reading of the report, where that problem is subordinate. I have not claimed and do not believe that any misrepresentation has been intentional. So far as Dr. Baird was misguided by the way in which I presented my results, I am willing to take the blame. I am mainly concerned, however, that his criticism should not prevent attention to this rare case. I feel that it has several important features which have not previously been studied after successful operations for complete congenital cataracts.

JAMES BURT MINER.

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WHEN a discussion has reached a certain stage, the main issues, which were clear-cut and definite at the outset, tend to become diffuse and obscure as a result of the controversial attitude assumed on both sides. I can not but feel that this stage has been reached in the present discussion. I shall endeavor, in my reply, to confine myself to points originally in dispute between

Dr. Miner and myself. And I shall take up these points in the order followed in the closing paragraph of the foregoing rejoinder.

1. Dr. Miner's first point,—which refers to the preliminary character of his paper,— has already been met.[1]

2. The review pointed out in detail that Dr. Miner's work was *neither thorough nor complete.* This criticism was not based upon a single group of experiments, as the rejoinder implies; the review specified some six other groups of experiments which were vitiated by the same defects.

3. Dr. Miner's report of his 'test on active touch' gives no insight into the degree of sensitivity possessed by his patient. The results of this test, as reported by the author, are, therefore, unmeaning.

4. It need scarcely be mentioned that practise tends to refine sensory function. No measurement of sensory capacity can have value unless the conditions of practise are specified. In describing his test in tonal discrimination, Dr. Miner made not the slightest reference to the degree or character of practise attained by his subject at the time when the test was made. His description leaves the reader wholly in doubt as to what was the degree of sensitivity possessed by his subject.[2]

5. Apropos of color discrimination, as Dr. Miner here confesses to an inaccuracy of phrasing, I can only regret a misunderstanding for which I am not in the least responsible.

6. The question of 'cataract' or 'cataracts' is a merely verbal issue. There is abundant authority in Ware and in Franz for the

[1] See this JOURNAL, Vol. III., No. 4, p. 101.

[2] The reader may be reminded that it comes to light only in Dr. Miner's second rejoinder that this test was made 'without practise' and 'with a minimum of fore-exercise.' That such initial records can not be regarded as furnishing a measure of discrimination is well illustrated by Professor Whipple's records, now referred to by Dr. Miner himself. As to the experimental data which he here brings into comparison, I need only call attention to the wide divergence of their conditions. Spearman and Myers deliberately planned to exclude, so far as possible, the influence of memory, in order that they might obtain a measurement of the purely sensory limen. The standard and the comparative tones were both sounded by the experimenter; the time interval between the two tones was reduced to a minimum,—two seconds in the one case, three quarters of a second in the other. Wissler had a wholly different problem, and he attacked it by a wholly different method. Here the standard tone was sounded, and then the instrument was mistuned and given into the hands of the subject, who was required to retune it to unison with the standard tone. Wissler himself says of his method: "The test for pitch is in many respects a test of memory" (*loc. cit.*, p. 33). I regret that in my former 'Reply' I ascribed to Dr. Miner certain results obtained in the Iowa laboratory *before* the publication of his paper. This was a mistake. But I must insist that it does not in the least affect the questions at issue.

form of expression which I employed; and the context in which I employed the term is neither equivocal nor misleading.[3]

7. As regards Dr. Miner's experiments in the visual perception of depth, the original paper stated that 'accurate tests were made upon her ability to discriminate distances with both eyes, compared with her monocular ability' (p. 114). And the results of these tests were expressed in quantitative terms, the liminal determinations being 6 cm. in the former case and 15 cm. in the latter. The author now points out that I was mistaken in supposing that he 'was interested in measuring her perception of depth'!

I quite agree with Dr. Miner's statement that a great deal of interest attaches to this case of acquired vision, and I have already pointed out that the investigation of such cases may throw light upon psychological problems. It is true that, in the present instance, upwards of three years elapsed between the date of the operation for cataract and the beginning of the examination of the patient. Yet it can not be doubted that the psychologist is in need of enlightenment upon just the topic which Dr. Miner chose for investigation.

J. W. BAIRD.

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WHILE I shall not prolong this controversy by meeting Dr. Baird's last statements, I would give the following references: In regard to Dr. Baird's second point, see this JOURNAL, Volume III., p. 46, line 26; and as to point '3,' see my original paper in the *Monograph Supplement, Psychological Review,* Volume VI., p. 108.

JAMES BURT MINER.

[3] In the sentence next succeeding the one in which Dr. Miner finds the objectionable expression, I twice referred to his patient as a 'blind person.' And I afterwards spoke of her as a 'born-blind' patient. In describing the experiments in which she acted as subject, I referred to her 'capacity to discriminate distance with both eyes' and spoke of her converging 'both eyes' upon the visual object. I pointed out that her binocular estimation was more than twice as accurate as her monocular estimation. I spoke of the fact that her two retinal images did not fuse into a single image (in the earlier experiments), and I stated that 'each retinal image persisted independently of the other and her visual objects appeared in duplicate.' Could the most obtuse reader understand from my review that Miss W. was blind in but a single eye before the operation, or that she saw with but a single eye after the operation? Wherein then have I 'misrepresented' the condition of his patient?

REVIEWS AND ABSTRACTS OF LITERATURE

I presupposti filosofici della nozione del diritto. GEORGIO DEL VECCHIO. Bologna: Ditta Nicola Zanichelli. 1905. Pp. 188.

Das Naturrecht und der Entwicklungsgedanke. GUGLIELMO SALVADORI. Leipzig: Theodor Weicher. 1905. Pp. viii + 108.

Owing, no doubt, to the continuity in political and social conditions, the theory of right is in Germany and Italy perhaps the topic in philosophy most seriously cultivated. Nevertheless the lack of agreement on fundamental points has caused the phrase 'the crisis in the philosophy of right' to become almost a technical term. According to Del Vecchio, and the host of writers cited by him, the confusion centers about the question, 'What is right?' Gierke ('Naturrecht und Deutsches Recht') puts it thus: "Am Eingange und am Ausgange der Rechtswissenschaft steht begrifflicherweise die Frage 'Was ist das Recht?' Der Rechtswissenschaft geht es nicht besser und nicht schlechter als ihren meisten Schwestern: in vielhundertjährigen Bemühen ist es ihr nicht gelungen eine endgültige und einwandfreie Antwort auf diese Frage zu finden, von deren Lösung doch für sie das Verständniss ihrerselbst abhängt. Nicht einmal zu einer allgemein anerkannten äusserlichen Definition, mit deren Hülfe die sichere Abgrenzung des Rechtsgebiets gegen andere Gebiete des menschlichen Gemeinlebens nach formellen Merkmalen möglich wäre hat sie es gebracht." Signor Del Vecchio will accordingly undertake to point the way out of the labyrinth for which the positive and empirical historical school is apparently chiefly responsible. Whether or not he succeeds depends upon whether the Kantian philosophy still has power to convince.

If we look to history to discover the nature of right, she exhibits a multitude of different and contradictory standards. Herodotos observed that every people upholds its own laws as the best ones; and this diversity of ideals has been an object of curious speculation ever since. It was a large item in the evidence appealed to by the Greek sophists and skeptics, and its recognition is a point of departure for perhaps every *Aufklärung*. Its first effect is in the direction of enlightened liberalism or skepticism, as seen in Montaigne and Pascal. Skepticism, however, does not dispose of the problem, but rather emphasizes it.

Accordingly this instability of notions of right led in antiquity to attempts to conceive a stable and authoritative criterion of justice, superior to the various systems of particular peoples, a criterion, namely, defined from the point of view of nature. The application of such a criterion would yield a system of 'natural' justice. But such a system is just one more system of right and adds itself to the already existing group. It neither overcomes the diversity which underlies the problem nor sums up the whole of right within itself. Hence the author concludes that the problem of 'natural right,' which is at present in his own country an object of such lively discussion, has nothing to do with the problem of his own present inquiry.

A doctrine closely related to that of 'natural justice' proceeds by distinguishing constant and variable elements. The purpose is really to give content to the idea of 'natural right.' Thus Aristotle distinguished what is *φύσει δίκαιον* from what is *νόμῳ* or *θέσει δίκαιον*. Those elements in the concept of justice which are derived from nature are common to the laws of all states. The criterion of empirical universality was the foundation of the Roman conception of *jus gentium*. But so far as the notion of natural right is made to depend upon a *consensus gentium*, its character of independent authority is denied. Moreover, an adequate concept of right must do justice to all of its object and not merely to the invariable part of it.

The philosophy of history has sought to systematize the diverse standards by exhibiting them as stages in an evolutionary journey which every nation is obliged to accomplish. The facts refuse, however, to follow one another in the ways that such theories demand. The theories are indeed grand imaginative constructions, but where there is gain of theoretical precision there is loss of truth.

More promising is the attempt to explain right by examining the relation it bears to its physical and social environment. This is the characteristically modern point of view, and here the influence of the more concrete evolutionary conceptions has been valuable, as well as the historical habit of mind cultivated by the Hegelian school, and the methods of historical positivism. In proportion, however, as this undertaking meets with success, the essence of the concept of right seems to be lost. Thus a survey of history gives us not right, but rights, and the author complains that modern speculation labors not so much to discover the 'pure substance' of the concept as its history and function. The reader wonders from the beginning why Signor Del Vecchio never thinks, apparently, of defining right in terms of function. He never once asks what right *does*, but always what it *is*. It is a little hard to feel the reality of his problem. Apparently the nature of right exists in imperatives of a certain type. The concept of right must, then, be a 'thou shalt' something; but what is it that 'thou shalt'? It is impossible to discover any one imperative which shall explain the particular actual imperatives as the universal explains the particular. But the formal concept of right, whatever content it may have, is logically prior to all concrete manifestations of it, and can not be understood from them. It is, therefore, a category of reason, making 'juridical experience' (p. 132) possible. Nevertheless, the idea of right is not a constructive spontaneity (*una potestà attiva, una gestaltende macht*, p. 181). How, then, it can be effective in the way demanded, the author will perhaps make clear in his next work. In this one he certainly oscillates between formal logic and Kantian epistemology. Yet to be quite fair, we must note that the title announces not a study of the concept of right, but of its 'philosophical presuppositions.'

Dr. Guglielmo Salvadori is more consistent. He criticizes the historical school in the same spirit and from the same point of view as Del Vecchio. He discusses the 'crisis' in the philosophy of right, and he

accounts for the same with a theory that is probably better than a brief review can make it appear. In the world of intellectual constructions there is an oscillation between a relatively Eleatic and a relatively Heracleitean position. The history of empiricism and positivism, since the days of great philosophical systems, has been a movement away from the interest in eternal principles toward an interest in the flux of empirical facts. In this flux of the matter of right the form of right has almost disappeared. It is accordingly high time to reassert the claims of the Eleatic position, in the definition of which a real philosophy of right must consist.

The weak side of the historical school has appeared in its lack of psychological insight. To say this is not to question the immense service rendered by the historical school. Dr. Salvadori does not wish to complain because historians stick faithfully to their own business. He does insist, however, that a philosophy of right must rest upon something which the historian, as such, is unable to supply. Right exists in the experience of individuals, as a factor in psychical human nature, and finds expression in will-activities. Psychology must accordingly ally itself with history in order to seek in human nature the real source of all ethical phenomena. This appeal to the science which studies the psychical individual is justified by the history of right which describes the primitive conception of right as a communistic consciousness, and the most developed conceptions as involving self-conscious individuality. And since man is not merely a psychical and a social, but a biological organism as well, biological science must come to reinforce history, psychology and sociology. All the time, however, the contrast between form and matter is evident, and while all the sciences of man contribute to the explanation of particular rights, the essence of right can be understood only as the operation of a mental spontaneity, as a typical activity of the 'schöpferischen Vernunft.'

By thus grounding the form of right in human nature, the basis is secured for a legitimate concept of natural right. This, however, does not give the form of right or state its essence. In order to arrive at a statement of the concept of natural right it is necessary to describe an ideal society, not by any *a priori* procedure, but by empirical observation. Such an ideal society is a community in which every member is in full possession of all the rights which issue from the principle of justice because these are the necessary presuppositions for every social advance. They constitute the freedom that must be guaranteed to the human individual in order that he may realize his highest ethical purposes; they are the right to freedom, the right to dignity and the right to complete individual development.

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The Principles of Descartes's Philosophy. BENEDICTUS DE SPINOZA. The philosopher's earliest work, translated from the Latin, with an introduction by Halbert Hains Britan, Ph.D. Chicago: The Open Court Publishing Co. 1905. Pp. lxxxi + 177.

This work of Spinoza's contains (1) an attempt to demonstrate certain portions of Descartes's 'Principia Philosophiæ' (not 'the principles of Descartes's philosophy,' as the translator puts it) *more geometrico,* and (2) an appendix containing certain 'Cogitata Metaphysica' of Spinoza's. It is prefaced by a lively and illuminating letter to the reader from Ludwig Meyer, to whom Spinoza had entrusted the work of publication. The first 'Part' of the main work is concerned with God, and the second with space, matter and motion; the third is a mere fragment. The sixty-two pages of 'Cogitata Metaphysica' contain a preliminary discussion of such subjects as being, essence, existence, necessity and eternity, a series of short chapters on the attributes of God, and a few pages at the end on immortality and the freedom of the will.

In these 'Cogitata' the geometrical method is dropped, but the topics are those of the ethic, and the question we naturally ask is, How far do they express Spinoza's own thought at the time and how far are they mere attempts to interpret Descartes? That Spinoza himself did not regard them as satisfactory expressions of his own thought is evident enough from his explicit statement that he holds 'the exact contrary to much that is there written,' for example, to the doctrine of the freedom of the will, and that the treatise is published in the hope that 'some of those who hold the foremost positions in my country will be found desirous of seeing the rest of my writings, which I acknowledge to be my own,' and make it possible to publish them without breaking the law. What these other writings were Dr. Britan does not tell us.

So much for Spinoza. Dr. Britan's introduction to the translation covers some eighty pages, and would have been much improved if he had condensed it into eight or ten. As to the translation itself, it is extremely slipshod, even where we have the right to expect the most painstaking care. Thus we find this definition at the beginning of Part I:

"VII. Substance, which is the immediate subject of extension, and of accidents, which presupposes forms of extension as figure, position and motion, etc., is called body (corpus)." This is hard to understand; but it would have been clear enough if the translator had not inserted a comma after 'accidents,' and had left 'presuppose' in the plural. The Latin reads, 'extensionis, et accidentium quae extensionem præsupponent.'

So with Definition IX. "When we say that something is contained in the nature of the thing itself or in its concept, it is the same as to affirm that this is true." But true of what? The Latin reads, 'idem est, ac si diceremus, id de ea re verum esse, sive de ipsa posse vere affirmari.'

Once more, in the discussion of Zeno's argument against motion (p. 73) we read: "For if you reply that a body does not move *in* the place it is, but *from* that place to another, he will ask whether it does not also

move through the intervening places. We reply by making a distinction —if through the term *was* we understand *to be at rest,* then we deny that the body was at any of the places through which it moved: but if by *was* existence is meant, then we say," etc. But where is this ambiguous term '*was*' on which the whole point depends? It ought to be in the clause which reads, 'whether it does not also move through the intervening places' (rogabit: An in locis intermediis non fuit?).

These are rather extreme examples, to be sure, but they do not give an unfair impression of the general character of the translation.

Some authors might be translated in this free and easy way without serious harm, but with Spinoza every word tells, and it is a pity that the translation should be less clean-cut and unambiguous than the original.

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A Reconciliation between Structural and Functional Psychology. MARY WHITON CALKINS. *The Psychological Review,* March, 1906. Pp. 61–81.

Professor Calkins's article is the presidential address at the Cambridge meeting of the American Psychological Association. It includes (1) a defense of the 'conscious self as basal fact in psychology,' (2) a reconciliation, through the offices of this self, of structural and functional psychology and (3) a description of consciousness in terms of structure and of personal relation. The object of the paper is 'to show that the two conceptions, structural and functional, are readily combined if only the basal fact of psychology be conceived as a conscious self, that is, as a self-being-conscious.' The possibility of the 'combination' or 'reconciliation' or 'harmony' of the two conceptions lies in the alleged fact that 'a self-being-conscious is not only analyzable into elements, but is also a complex of relations to its environment, social and physical.'

The most noteworthy feature in the address is the new position taken by the author with regard to the doctrine of 'selves.' In earlier expositions, the 'psychology of selves' has been made coordinate with 'structural' or 'process' psychology. Any mental experience whatsoever, it has been urged, may be treated either as an idea or event 'without any reference at all to any self,' or as an experience of a 'self' or 'ego.'[1] Now, however, Miss Calkins declares that 'in spite of the abstract possibility of conceiving consciousness as a series of ideas and psychology as the science of this stream of ideas, I am none the less convinced that not the idea, but the self, should be taken as the basal fact of psychology.' This conviction is defended in two ways: (1) 'the idea is itself an abstraction which invariably implies a self' and (2) no description in terms of process gives 'a full and adequate account of actual conscious experience.' These defects of process psychology may be eliminated by sub-

[1] Cf. 'An Introduction to Psychology,' 1901, pp. v, 12, 149 ff.; also, 'Der doppelte Standpunkt in der Psychologie,' 1905.

suming both structural and functional under self psychology. The author is careful to repeat that by 'self as fundamental fact of psychology' she means neither the psychophysical organism nor the philosophical or sociological self, but the 'psychological self'—'an object of immediate experience.' Thus conceived, the self is the instrument of the promised reconciliation. The argument runs as follows: First, self psychology is structural psychology because it employs the method of analysis into elements, which is the essential feature of structural psychology. Secondly, self psychology is functional psychology because it regards and sets a value to the experienced relations of self to environment. To be sure, functional psychology, in so far as it forms an alliance with biology, alienates itself from the more generic form; but, in so far as it remains true to its 'cardinal conception,' that of 'consciousness as involving internal relation to environment,' it falls naturally under self psychology, and the reconciliation is complete. "Consciousness, which always implies a conscious self, is a complex alike of structural elements and of relations of self to environment."

It is, perhaps, captious to suggest that, in the proposed settlement of differences, the arbiter rather absorbs than 'reconciles' the disputants. Assuming, however, that the latter were willing to be devoured in the interests of harmony, it would behoove outsiders to rest content were it not that the quarrel—if quarrel there really be—is far more a matter of psychologists than of psychologies. The dispute, so far as dispute exists, springs more from temperamental diversity and from disparity of interest than from any incompatibility of structure and function. Practically all psychologists would, I believe, if the distinction in question became general, acknowledge the validity and value, under some form, of both procedures. What is of primary importance is not that the differences of structure and function should be moderated by a *tertium quid,* but that the investigator should know when he is dealing directly with consciousness itself and when with relationships, dependencies and functions, and that he should be able, in either case, to use appropriate instruments and methods.

To revert to the 'reconciliation' itself, it may fairly be asked whether the assumption of a 'self' serves to establish more than a formal harmony of the alleged differences. Is it not as simple and as natural to regard conscious experience from both points of view under discussion, but without reference to the self? Moreover, can the self, once discovered and admitted, as 'basal fact,' into psychology, be made to stand in any *vital* relation to both structure and function? The relationship with the former is not unlike that of foster-parentage. Structural analysis has not, as a matter of fact, usually been regarded as analysis of the 'self,' nor does the method depend for its existence upon the 'self.' At most (as the argument of the address concedes) the self is only supplementary: it can adopt and protect and improve, but it can not originate. To functional psychology, on the other hand, the psychology of selves stands in a much more intimate relation. The parentage in this case is real. Indeed.

we may pass beyond the figure of generation and say that self psychology is a *species* of functional psychology. But we must not confuse the species with the genus. There is also the psychophysical kind, which, however much its 'biological excrescences' need 'pruning,' is to be reckoned with. Then there is the function that springs, independently of the 'self,' from the mental process as such or from the organized group of processes. Again, function may imply not conscious relation of self to self, but 'transcendence' in general or, finally, the reaction of consciousness upon itself (a kind of function common, *e. g.*, in the writings of Stout and Lipps). These types of function are, in all probability, not all coordinate, and the list lays no claim to finality, but the very fact that the word 'functional' is employed in psychology in so many different senses should serve as a warning against hasty generalizations regarding the use of the term.

Despite the author's intentions, the address seems, then, to the present writer, strongly to indicate that self psychology is essentially a type of the psychology of function; that it is, therefore, as in the earlier expositions it appeared to be, *coordinate with* and not inclusive of structural psychology. Thus regarded, it is, without doubt, in a position to perform —or, better, to complete—an important and timely service for psychology, namely, to supply a systematic and *psychological* account of the functions and relationships of the conscious individual.

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CORNELL UNIVERSITY.

JOURNALS AND NEW BOOKS

AMERICAN JOURNAL OF PSYCHOLOGY. January, 1906, Vol. XVII., No. 1. *The Psychology of the Simple Arithmetical Processes: A Study of Certain Habits of Attention and Association* (pp. 1–37): CHARLES E. BROWNE. – Attention is involved in the data and the result of a problem, association in the process of solving it. In addition and subtraction the association is less rapid and direct, and is generally accompanied by a tendency to articulation on account of the derivation of these processes from simple counting. In multiplication and division the association is more rapid and direct, the tendency to articulation disappears, and the result is accompanied by a sense of accuracy. Pedagogical inferences. *The Time of Some Mental Processes in the Retardation and Excitement of Insanity* (pp. 38–68): SHEPHERD IVORY FRANZ. – Experiments made on normal, depressed and excited subjects by taking their time for certain simple processes show that in the maniacal condition there is no increase of motor ability, but simply a diffusion of the motor impulse, and that in the depressed condition the retardation takes place in the peripheral organs rather than in the central. *Acquisition of Written Language by Primitive Peoples* (pp. 69–80): ALEXANDER F. CHAMBERLAIN. – The various attempts made by white men and by Indians

to reduce the different Indian languages to writing are interesting in themselves and valuable for the light they may throw upon the general problems of reading and writing. Bibliography. *An Experimental Examination of the Phenomena usually Attributed to Fluctuations of Attention* (pp. 81–120): C. E. FERREE. – A long series of careful experiments show that the changes in the intensity of the visual field commonly attributed to fluctuations of attention are in reality due to interruptions in adaptation caused by eye movements. *A Comparison of Methods for the Determinations of Ideational Type* (pp. 121–126): ALMA BELL and LORETTA MUCKENHAUPT. – The different methods used agree, on the whole, in their results, though the deviations seem to point to imperfections in some or all of the methods. *The Kinesthetic Element in Endophasia and Auditory Hallucination* (pp. 127–133): CLARA HARRISON TOWN. – The view that thinking is carried on mostly by the aid of auditory and motor images, *i. e.*, is a sort of an internal speech, is confirmed by some cases of insanity, in which the internal thinking becomes external and hallucinatory. *The Negative Aspect of Hallucinations* (pp. 134–136): CLARA HARRISON TOWN. – On account of the negative aspect of hallucinations—their power to inhibit sensations from the periphery—Dr. Stoddard explained them as due to a dissociation of the sensory centers from the sense organs, but since the author found cases of hallucination lacking the negative aspect she regards them as due rather to a concentration and diminution of attention. *New Books. Notes and News: The Annual Meeting of the American Psychological Association.*

ANNALEN DER NATURPHILOSOPHIE. December, 1905, Band V., Heft 1. *Grundzüge des modernen Seelenlebens in Deutschland* (pp. 1–50): K. LAMPRECHT. – From 1500 to 1800 A.D., individualism, having expelled the medieval view of man as a mere member of a genus, glorified understanding at the expense of the unifying influence of affection and imagination. Its two stages and its workings in many fields. The development of subjectivism: new emotions arise from introspection, the love of nature, morbidity; psychology arises and free personality is achieved, finding in itself the source of objective laws and seeking, in sports and dilettantism, activities beyond those of mere necessity. Kant's criticism of the mechanical conception and Darwin's demonstration of the mutability of species alike magnify personality because they emphasize life. Tolerance, democracy and cosmopolitanism, are based on the one spirit of subjectivism. *Ueber Harmonie im Weltraum* (pp. 51–110): V. GOLDSCHMIDT. – The distances of the planets from the sun result from the properties of a condensing ball of gas, and also follow the 'law of complication,' which prevails also in vibrating chords, in all tonal harmonics and in the development of colors and crystals, and sense and thought. So, also, the sizes of the planets follow this law, as do the satellites in their relations to their planets. *Die zwei Hauptprobleme der Wirtschaftswissenschaft* (pp. 111–118) J. ŽMAVE. – The problems are those of value and of labor, of which the latter is altogether fundamental. *Die Erkennt-*

niskritische Betrachtungsweise in der Biologie unserer Zeit (pp. 119–133): P. OPPENHEIM. – The problem of all naturalistic speculation is the explanation of some *dualism;* in biology the monistic reality can only be explained dualistically, whether we regard the dualism of reality and the way in which it is experienced, or that of the modes of observation, of which the author distinguishes four. *Neue Bücher* (pp. 134–136): H. DINGER, *Dramaturgie als Wissenschaft.* – W. O.

Abraham and Langevin. *Les Quantités élementaires d'Électricité; Ions, Électrons, Corpuscles.* Mémoires réunis. Two volumes. Paris: Gauthier-Villars et Fils. 1905. Pp. xvi + 1138. 35 fr.

Bolin, Wilhelm. *Pierre Bayle, sein Leben und seine Schriften.* Stuttgart: Frommann. 1905. Pp. 111. 2 M.

Bonucci, Alessandro. *La derogabilità del diritto naturale nella scolastica.* Perugia: Vincenzo Bartelli. 1906. Pp. 292.

D'Alfonso, N. R. *I limiti dell esperimento nella psicologia.* Rome: Loescher and Co. 1905. Pp. 21.

Freytag, W. *Entwicklung der griechischen Erekenntnistheorie bis Aristoteles, in ihren Grunrzügen dargestellt.* Halle a/S: Max Neimeyer. 1905. Pp. iv + 126.

Jastrow, Joseph. *The Subconscious.* Boston and New York: Houghton, Mifflin and Co. 1906. Pp. ix + 549. $2.50.

Kuberka, Felix. *Kants Lehre von der Sinnlichkeit.* Gekrönte Preisschrift der Krug-Stiftung der Univ. Halle-Wittenberg. Halle a/S: Kaemmerer und Co. 1905. Pp. viii + 146. 2 M.

NOTES AND NEWS

DR. WILLIAM TURNER, a former professor at the St. Paul Seminary and at present in charge of St. Luke's parish in St. Paul during the absence of Rev. Ambrose McNulty, has been appointed professor of philosophy at the Catholic University of Washington, D. C., and he will begin his new duties on October 1. Dr. Turner was born in Limerick, Ireland. He was a student at the Jesuit college of his native city, and at the age of seventeen he was graduated from the Royal University, in Dublin, taking second honors in philosophy. He went to Rome, and while pursuing his theological course also studied philosophy, and was awarded the Benemerenti medal, given for special elections in philosophy. He next went to Paris and studied at the Sorbonne until 1894, when he went to St. Paul, and for ten years was professor of philosophy in St. Paul's Seminary. Dr. Turner is the author of the well-known 'History of Philosophy.'

THE following is from *Nature,* April 19: "Sir Thomas Browne, the author of 'Religio Medici,' who lived at Norwich in the middle of the seventeenth century, was buried in the church of St. Peter Mancroft

during the early part of the last century. It is believed that his skull was abstracted from the grave and is now preserved at the Norfolk and Norwich Hospital. We learn from the *Times* that recently there has been a considerable expression of opinion in Norwich that the skull ought to be returned to the tomb whence it was taken. The hospital governors on Saturday unanimously passed a resolution agreeing to this course, on condition that the tomb shall be opened in the presence of representatives of the hospital with the view of satisfying them that the remains therein are without a skull."

IT is proposed to erect a memorial to the late Mr. Frederic W. H. Myers in the chapel of Cheltenham College, his old school. Mr. Waterfield writes, "The memorial will take the form of a wall picture in an arched recess surmounting some emblematic wood-carving, the whole designed to illustrate the Resurrection and the Doctrine of Immortality, which, during the best years of his life, Mr. Myers strove so earnestly to make more real to his fellow men." Contributions may be sent to Mr. F. J. Cape, The College, Cheltenham.

IN a paper read by Baron de Haulleville before the Colonial Club, of Antwerp, on the religions of the natives of the Congo, the writer declared that for the negroes water represents all that is mysterious, just as fire does to the eastern imagination. Tizambi, the supreme being, lives below the waters and cares nothing about the welfare of his followers. The negroes feel confirmed in their belief in the mysterious power of water by the fact that white men came to them across the ocean and up the Congo.

THE Cambridge University Press has in preparation 'The Axioms of Geometry,' by Dr. A. N. Whitehead. Swan-Sonnenscheim and Co. promise 'Thoughts and Things—A Genetic Study of Logical Process,' in two volumes, by Professor J. Mark Baldwin, Vol. I., 'Theory of Knowledge, Functional Logic,' Vol. II., 'Theory of Reality, Real Logic.'

THE Ravizza prize of two thousand five hundred francs is offered for the best study of 'The Psychology of the Normal and of the Abnormal Child in Relation to Education.' As Italians only are admitted to the competition, we do not publish the conditions.

AT the last meeting of the French Institute ten thousand francs (from the Prix Debrousse of thirty thousand francs) was devoted to publishing the works of Leibniz.

PROFESSOR FRANK THILLY, professor of philosophy in Princeton University, has been made professor of philosophy in the Sage School of Philosophy of Cornell University, succeeding Professor E. B. McGilvary, who was last year called to the University of Wisconsin.

PROFESSOR WILHELM OSTWALD, of Leipzig, has been elected a foreign member of the Danish Academy of Sciences. According to the *New York Times,* he has resigned the professorship of chemistry at the University of Leipzig and will establish a private laboratory.

DR. B. H. BODE has been appointed assistant professor in philosophy at the University of Wisconsin.

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Volume III. No. 6. March 15, 1906.

The Moral Individual. J. DASHIELL STOOPS.
On 'Feeling.' GEORGE M. DUNCAN.
Societies: The Fourteenth Annual Meeting of the American Psychological Association. VIVIAN A. C. HENMON.
Reviews and Abstracts of Literature. Journals and New Books. Notes and News.

Volume III. No. 7. March 29, 1906.

Recent Discussion of Feeling. JAMES ROWLAND ANGELL.
Cognitive Thought and Immediate Experience. J. A. LEIGHTON.
Discussion: The Quarrel about Transcendency. W. H. SHELDON.
Reviews and Abstracts of Literature. Journals and New Books. Notes and News.

Volume III. No. 8. April 12, 1906.

The Ground of the Validity of Knowledge. EDWARD G. SPAULDING.
Discussion: Professor Calkins's Mediation. MARY S. CASE.
Reviews and Abstracts of Literature. Journals and New Books. Notes and News.

Volume III. No. 9. April 26, 1906.

Thought Revealed as a Feeling Process in Introspection. E. A. NORRIS.
Memory Types of Colorado Pupils. WILL GRANT CHAMBERS.
Discussion: Thought and Immediacy. F. C. S. SCHILLER.
A Note to Professor Angell. HENRY RUTGERS MARSHALL.
Reviews and Abstracts of Literature. Journals and New Books. Notes and News.

Volume III. No. 10. May 10, 1906.

Reality as Experience. JOHN DEWEY.
The Ground of the Validity of Knowledge. Part II. EDWARD G. SPAULDING.
Societies: Section of Anthropology and Psychology of the New York Academy of Sciences. R. S. WOODWORTH.
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